SCIENCE

FRIDAY, JANUARY 13, 1888.

WE HAVE THE GREATEST SYMPATHY with those educators who are endeavoring to secure the introduction of science-teaching into the public schools. We would advocate this addition to the present curriculum, not only because of the interest and value of scientific knowledge as such, but because of its value as general information. A great deal of that which is incorporated under the head of elementary science is really general information, and as such should be in the possession of every child in the grammar-schools of the country. We regard the little book entitled 'Introductory Steps in Science,' by the late Paul Bert, as invaluable in this connection, and the English translation should be in every school. Nowhere else are the facts stated as simply, as clearly, and as comprehensively as in this little book. That this subject is beginning to attract the attention which it deserves, is evident. At a recent meeting of the American Society of Naturalists at New Haven it was elaborately and enthusiastically discussed, and now a valuable impetus is to be given to this movement among the teachers themselves by the proposition of the *Academy*, which is one of the best journals of secondary education published in this or any other country, to give a prize of fifty dollars for the best essay on 'Science in Secondary Schools.' The effect of this offer will be to stimulate the teachers of the country to investigate the subject in its practical bearing. It is announced that the committee of awards will give no weight to essays that are merely arguments in favor of science-teaching. This is as it should be, for, unless this condition was made, the majority of the essays would be given over to the threshing of old straw. Contestants are requested to confine themselves simply to the practical exposition of the results arrived at in the schoolroom, and to the best means of obtaining these results. The competition is open to all persons, without regard, as the announcement puts it, "to age, sex, color, or previous condition of servitude," and no paper is to exceed five thousand words in length. All essays must be received at the office of the *Academy*, Syracuse, N.Y., on or before March 15, 1888. We cordially recommend this competition to all persons interested in science-teaching. It gives them an excellent opportunity to be of practical service to the public-school system of the country.

THE MEETING of the Engineers' Club of Philadelphia, Dec. 17, was another instance of the advantage of providing something to eat and smoke at scientific meetings. The secretary, in his report, states that he is glad to be able to announce that both members and guests seem to have been much pleased with their little entertainment. Whereas the usual attendance at the meetings may have varied from a dozen to twenty, the attendance at this meeting amounted to something like three hundred and forty-two. It was not possible to determine exactly. In this case there was no speech-making, or any attempt to introduce any feature which might have deprived the affair of an entirely informal and purely sociable character; but it is believed that the entertainment will be of permanent and substantial benefit to the club. The decline of the old scientific meetings is well illustrated in those held, or attempted to be held, by one of the oldest scientific associations of the country. This association, although it has maintained its existence for more than a hundred years, and has accumulated a library of scientific periodicals and Transactions of societies which is excelled by but one or two in this country, has found it impossible, since the opening of the present season last October, to bring together a sufficient number to form a quorum for the transaction of any business: in other words, no new members have been elected, because fifteen out of the two hundred members of this society had never been sufficiently of one mind to attend its meetings, which are held in a building easily accessible to a very large proportion of them. This society has, as well, tried the social experiment once or twice, and with promising success; but it certainly seems, that, with the differentiation of the interest and work of scientific men, many of the older general scientific societies must develop some new field in which they may be of service. In large degree they are now publication societies, but, as is well known, there is a great disadvantage in the publication in one volume of a vast mass of heterogeneous material. It frequently amounts to a mere burying of the results.

MODERN-LANGUAGE ASSOCIATION.

THE fifth annual convention of the Modern-Language Association was held under the auspices of the University of Pennsylvania on Wednesday, Thursday, and Friday, Dec. 28–30. The large attendance of delegates, chiefly from the East and South, but some also from the West, was gratifying as indicative of the steady growth of the organization. But all who came were amply repaid, for the proceedings were both interesting and instructive. The sessions were opened on Wednesday evening with an address of welcome by Provost Pepper of the university, who was followed by Prof. James MacAllister, superintendent of the public schools of Philadelphia. Professor MacAllister spoke on the place of modern literature in the education of our time. It was a discussion of the topic which at present is engaging the attention of pedagogues all over the world, whether the classics should continue to remain the basis of a liberal education or not. Professor MacAllister ranged himself clearly and openly on the side of those who favored the substitution of modern literatures as such a basis in place of the study of Latin and Greek. After tracing the origin of the system of education which was still in vogue in all parts of the universe fifty years ago, to the revival of classical learning in the days of the Renaissance, he argued, that while it was natural for the men of the fifteenth century to go to the classics for satisfying their sense of beauty and their desire for knowledge, for it was the Latin and Greek authors who had set these aspirations and desires in motion, there is no sufficient reason why we, in our days, should go to the same fountain for quenching our thirst. With the achievements of modern nations in the realms of philosophy, poetry, science, and literature, it is strange that we should continue to train the intellect and to stimulate the heart almost exclusively upon works access to which is possible only after prolonged and laborious study of the languages in which they are treasured up. It is true that much has been done during the past decades towards dethroning the classics from the supreme rulership which they formerly exercised. After a good deal of fighting, science has found a place in our system of education, and it is conceded that any scheme of instruction is incomplete that does not provide for the teaching of modern languages; but the controversy is by no means ended. Professor MacAllister claimed that the modern literatures of the world contained all the elements necessary for attaining the aim of culture, which is to "know ourselves and the world," and that sooner or later they must be given the first place in the intellectual culture of our time, and be made the chief instruments of literary training in the schools.

On Thursday morning, after the transaction of routine business, the reading and discussion of papers began, and continued, with an intermission of one hour at noon, until late in the afternoon. The papers on this and the following day were of two kinds, — some of a technical character, giving the results of detailed investigations of some special subject; and others of a more general character,

dealing chiefly with questions affecting the teaching of modern languages. Among the latter was a suggestive treatment of the modern-language seminary system, by Prof. H. S. White of Cornell University.

The purpose of the seminary is to guide the student towards independent investigation; but, in order to do its work properly, the student must first have gone through a preliminary training of no inconsiderable character; and, in the second place, the seminary must be well equipped with the standard editions of the best authors, pamphlets, manuscripts, documents, photographic reproductions of important scenes and monuments, epigraphical material, and the like. In the method of teaching, all study of authors must be based upon a study of the times in which an author wrote.

Professor Kroeh of the Stevens Institute presented a paper on methods of teaching modern languages. After enumerating the various methods which have found followers, and discussing their merits and disadvantages, he pronounced himself in favor of the so-called 'natural method.' The basis of all languages, whether literary or scientific, is the phraseology of every-day life, and this can be learned only by imitation. The 'natural method' proceeds on this principle. But the imperfect training of the ear, or rather the total absence of such training in our schools, causes great difficulties in carrying out this method. The education of young people is still conducted almost exclusively through the eye by means of books. There is so little oral instruction, that the pupils not only do not hear accurately, but have to learn the art of paying attention.

One of the best papers, partaking of this general character, was that by Prof. Albert Smyth of the Philadelphia High School, on American literature in the classroom. "It is certainly discreditable to us that we have done so little towards a faithful and affectionate study of what is purely native and national in our American writings. The text-books, with one or two exceptions, designated for use in schools, show no critical utility and no sense of proportion. This is due to the neglect of the study in the higher classrooms. There are two objects to be reached by a proper attention to this branch: in the first place, it may be highly serviceable in education, because it, more than any other, admits of a complete severance of literature from philology; second, the study would ultimately assist in the development of that literature, and would discipline in it the critical faculty, for it must be admitted that America has not participated in the splendid progress of criticism in Europe during the last twenty years. We are poorest of all in criticism, and when we think of the high service the trained and faithful interpreters of poetry render to a nation, it would be hard for us to overrate the good results that may follow the extension of the English curriculum to include the genesis and brief history of American authorship. It is our precious property to hold the literature of our nation true to the higher ideals of life and its purpose."

There were two papers discussing dialects, by Professor Primer of Charleston, and Sheldon of Harvard University. The former dealt with 'Charleston's provincialisms,' also called 'Charlestonese' by the people in the South: the latter gave specimens of a Canadian French dialect spoken in Maine. In discussing the latter, Professor Elliott of the Johns Hopkins University spoke of the importance of such investigations at the present moment. In a generation or two, all traces of these old dialectical variations, whether in Canada or the South, will probably have disappeared, and, unless they are now accurately noted down from the lips of those speaking these dialects, they will be lost forever to scholars and students of dialectology.

Among the technical papers may be mentioned Professor Collitz's (of Bryn Mawr College) exhaustive essay on the origin of the so-called weak verbs in the Teutonic languages, and Dr. Goebel's review of Paul's 'Principles of *Sprachgeschichte*.' Professor Tolman of Ripon College, Wisconsin, read a paper on the style of Anglo-Saxon poetry. He compares the poetry to "a spirited horse, who takes a few bounds forward, and then stands prancing." Anglo-Saxon poetry is always more than lively, it is intense. Among the peculiarities of Anglo-Saxon poetry, the great scarcity of similes is worthy of note. On the other hand, as a kind of compensation for this defect, we have an abundance of striking poetical synonymes. For instance, the ocean is called such names as 'the whale's home,' 'the fish's bath,' 'the swan's road,' 'the sail road,' 'the course of the flood,s. 'the cup of the waves.' Another striking feature of this poetry is the idealization of the sensual and common. In conclusion, Professor Tolman said that he doubted whether the world has ever seen a purer literature than that covered by Anglo-Saxon poetry.

The proceedings were enlivened by spirited discussions. Before the sessions closed, the convention heard the report of a committee appointed to consider the question of petitioning Congress for a removal of the tariff on foreign books. The committee favored a personal presentation of the subject before the proper Congressional committee, and gave the following as the reasons why the tariff should be removed: —

"The revenue derived from the tax is very inconsiderable, and is wholly unnecessary to the maintenance of government. The theory of protection to domestic industry does not enter into the question. American authors do not desire protection for the reason that books are not merchandise and do not compete with one another. Buyers of books are not governed as ordinary buyers by consideration of price, but by consideration of taste or personal fancy and of special availability for special ends. One book is bought in preference to another, not because it is cheaper, but because it is better. The tax upon foreign books bears heavily upon the class which is least able to meet the financial burden; viz., the professors, teachers, and students. Foreign works, whether in English, French, or German, are absolutely indispensable to these people, and we regard such a tax as is now put upon them as directly harmful to the cause of knowledge and culture of our country. By this book-tariff the 'tools' of our profession are made unnecessarily expensive."

After the election of officers, headed by James Russell Lowell as president, the association adjourned, to meet again during the current year in Cincinnati. The delegates were entertained during their stay by the Historical Association, the Penn Club, and the University of Pennsylvania.

SCARLET-FEVER REPORT.[1] — III.

R. STANSBURY SUTTON, M.D., LL.D., Pittsburgh, Penn., says, "I know to a certainty, that, when I was a general practitioner, I conveyed the disease from a babe who died, to an adult woman who recovered. I recall an instance where the little patient played with the cat. The cat carried the infection to other children in a neighboring house, they having caught and played with it, stroking its fur."

Adolph Koenig, M.D., Pittsburgh, Penn., cites the case of a physician who visited his home during his attendance on a course of medical lectures, some hundreds of miles distant. While at home he came in contact with a younger brother suffering from scarlet-fever. About one week after his return to college he was attacked with scarlatinous sore throat, accompanied with fever, and lasting a number of days. He is decidedly in favor of compulsory reports to be made to boards of health, the State to assume the expense; and the legally qualified physician is the only person capable of making such a report. Laymen would undoubtedly often confound other eruptive fever with scarlet-fever.

J. F. Kennedy, M.D., Des Moines, Io., secretary State Board of Health, reports a fatal case of scarlet-fever in the family of a washerwoman, traced to infected clothing. He regards the disease as communicable from the patient until desquamation has fully taken place, the patient thoroughly bathed, and his person and clothing disinfected. From thirty to thirty-five days would be about the period of danger, dating from the beginning of the attack. A case was reported to the State Board of Health in which scarlet-fever was alleged to have broken out in a family, having been contracted from a dress which had been worn two years previously by a child who at that time died of the fever. Attending physicians should be required to report all the facts connected with each case of the disease that comes under their care, especially the cause and source of infection. Dr. Kennedy says, "I have for several years, in cases of scarlet-fever and diphtheria, used as a prophylactic zinc ferri chlor. and glycerine, equal parts, and giving according to age, to all exposed, from ten to forty drops in water every three or

[1] Continued from *Science* of Jan. 6, 1888.

four hours. I give the iron as a germicide, believing it equally effective in scarlet-fever and diphtheria. I would respectfully refer you to 'Health Exhibition Literature' of the Epidemiological Society of Great Britain, the publications of the American Public Health Association, to reports of State boards of health, to Ziemsen's Encyclopædia on scarlatina, and to articles in the *American Journal of Medical Science*.

Jerome Cochran, M.D., Montgomery, Ala., State health-officer, says, "Our law requires all cases to be reported (1) by the physician in charge; (2) if there is no physician, by the head of the family. We have boards of health in all of our counties, and isolation and disinfection are practised. Isolation and disinfection properly done would go far to prevent its spread. Absolute isolation would, I think, prevent it absolutely."

J. W. Parsons, M.D., Portsmouth, N.H., believes that scarlet-fever has arisen *de novo*, on the ground that after due inquiry no source of infection could be discovered. He thinks that heads of families, as being most interested, should be required to make reports of cases to health boards, and not physicians, who already have enough of such gratuitous work to perform.

George H. Rohé, M.D., professor of dermatology in the College of Physicians and Surgeons, Baltimore, says, "I have never seen any evidence which seemed to me to establish the *de novo* origin of scarlet-fever at the present time. In 1877, I was medical attendant to a poor family, in which there were three children, — a boy of eleven, a girl about eight, and another younger child. The boy contracted scarlet-fever, it was supposed, at school. The other children were both attacked a few days later. Two out of the three died. In this case isolation was impracticable, as the family (of six) lived in two rooms. In 1882 an almost identically similar instance occurred in my practice. A girl of six was taken ill with scarlet-fever, and several days thereafter two younger children, aged four and two respectively, were also attacked within twenty-four hours of each other. The youngest child succumbed to the disease. Isolation was attempted in this instance when the first child was taken sick, but the stupidity of the parents rendered all attempts at prevention nugatory."

Dr. Rohé accepts the general professional opinion that from six to eight weeks should be allowed to pass before the period of danger of infection can be said to be over. He is convinced that thorough and repeated disinfection of the surface of the patient would decidedly reduce the period of infectiveness of the patient, and has so expressed himself in his address on State medicine (see *Journal American Medical Association*, July 2, 1887). He further says, "All cases of scarlet-fever (and all other infectious diseases) should be promptly reported, as soon as the diagnosis is made, to the health authorities. These reports should be made by the attending physician, in order (1) to have a prompt report, (2) to avoid false and malicious accusations, which would be easy if this duty were left to irresponsible persons. Further, a neglect of this duty, if it devolved upon the householder, might cause disastrous results, and afterward give rise to disputes and questions of veracity between the physician and the patient's family. This duty of compulsory notification, if imposed upon physicians, should, however, be made as easy as possible, and should not involve any expense to the practitioner. The question of compensation for such service is one open to debate. No member of the legal profession, whether an official or not, will perform any service for the State without exacting a fee. There is no equitable reason why a physician should be required to act otherwise. Both sickness and death notices furnished to the authorities should be paid for by the latter. It seems to me the duties of boards of health, if notified of the existence of contagious diseases, and when empowered by law, would be to secure the isolation of the patient, disinfection of apartments after recovery or death, private funerals, notification of school-officers if children from the infected house are attending school, supplying disinfectants, and, whenever necessary, invoke the aid of school authorities to close schools. In addition to the public measures mentioned, personal disinfection of the body of the patient, by daily sponging with an effective solution of chlorinated soda or thymol, or inunction with a disinfecting unguent or oil, with immediate disinfection of all discharges and bed-linen, would, I feel sure, result in a marked restriction in the disease. I believe isolation hospitals would aid materially in restricting this disease. Proper instruction of the public (and, I may add, of the medical profession) would be a strong help to practical sanitarians. There are even health officials known to me who might profit from such instruction. I do not think the prophylactic administration of remedies would accomplish much good. Avoiding contact with the infective material is the best and surest means of prevention." For information touching the communication of bovine scarlet-fever to man, Dr. Rohé refers to the reports of Mr. W. H. Power and Dr. E. Klein in the *Practitioner*.

T. B. Heimstreet, M.D., Troy, N.Y., thinks that cases of scarlet-fever should be reported to health boards by medical attendants, and that these boards should prevent the attendance at school of other children of the same family in which the disease exists, and should disinfect the apartments, etc.

George Glenn Wood, M.D., Muncy, Penn., writes, "My plan of preventing the spread of scarlet-fever would be to establish one or more scarlet-fever hospitals, according to size of city, on the same plan that small-pox is managed. Inasmuch as the large cities are the usual hot-beds for this, as all infectious diseases, and its suppression there would mean the escape of rural cities and towns, the proper management would be to stamp it out at the former places. If, then, scarlet-fever patients were instantly removed, and quarantined in such special hospitals, there attended by the family physician if desired, and nursed by parent, friends, or professional nurse, advantages would occur not only to patient, but to other members of same family, and the public at large. Of course, to be effectual, the whole matter must be compulsory."

Lincoln R. Stone, M.D., Newton, Mass., says, "I can hardly say that cases can arise *de novo*, but a few years ago, in July, in a farmhouse during haying, a case occurred where no case had been known for years. There had been no intercourse with other people, no other case known in town. The house was situated on top of a high hill, half a mile from any family. The patient was a young child about three years old. There was no other case in the house, though young children in the family. Most careful inquiry could throw no light on the case: it seemed almost *de novo*." He reports a case where a blanket, used by a child before and during an attack of scarlet-fever, by some accident or carelessness, was not cleansed or destroyed after recovery, and a child, a relative, visiting, played with the blanket and had a severe attack.

D. W. Hand, M.D., St. Paul, Minn., member of the State Board of Health, thinks that placards should be placed on the houses where scarlet-fever exists, so as to give the public notice of the infection. He knows of several instances where strict isolation and disinfection have confined the disease to one child in a family.

A. J. Howe, M.D., Cincinnati, O., in reference to the *de novo* origin of scarlet-fever, says, "I do so believe, but may be mistaken. My belief is based on the fact that typhus, erysipelas, and diphtheria do arise *de novo*, under influences which develop zymotic poison. Possibly scarlet-fever virus is too strictly specific to come from any thing but the scarlatina germ." Dr. Howe relates the following incident: "A gentleman of my acquaintance, living in the country, brought a child, a boy five years old, from a city fifty miles distant. On the way, when near home, he stopped at a schoolhouse a few minutes during recess, and several of the pupils gathered around the little stranger. The next morning I was professionally called to the child, and found him violently sick with scarlet-fever. In eight days from that time, thirteen out of twenty-seven of the school-children were down with scarlet-fever. There had not been a scarlatina case within five miles for three years." He recalls another instance in which a wadded hood, that of a child which died of scarlet-fever, was the bearer of the disease to a child in the country, to whom the garment was given. In this case the article retained the infective virus two months, — March and April.

J. Howard Morgan, M.D., Westerly, R.I., reports, "I have now under care eleven cases in one family, who are convalescing from scarlet-fever of rather mild type, the first three of which (viz., the youngest three of the family) began to sicken seven days after the coming of their grandmother to visit the family, from a place some six miles away, where she had been attending for a week or two another grandchild who had 'sore throat and the same sort of rash,'

but was not sick enough to necessitate calling a physician. The grandmother wore nearly the same clothing while on her visit that she had when attending this previous case. The other eight cases of the eleven probably took the disease from the first three, since, owing to the size of family and their circumstances, satisfactory isolation could not be had; and I know of no other cases in the vicinity."

Dr. Morgan recommends to disinfect discharges from bowels, bladder, and throat by adding an equal volume of solution of corrosive sublimate (1 to 500); to anoint the skin daily during desquamation stage, so as to diminish the risk from fine scales of epidermis floating in the air; to bathe frequently the skin during that stage, and to disinfect the water so used by adding an equal volume of the corrosive-sublimate solution; to disinfect bedding, clothing, etc., by soaking in a solution of corrosive sublimate (1 to 1,000) or by prolonged boiling in water; to disinfect rooms, etc., by burning dust and sweepings, washing wood-work, etc., with corrosive-sublimate solution (.1 to 1,000), and by thorough fumigation with sulphur-fumes finally; to forbid nurses or members of the family attending the sick to mingle with others without first disinfecting their hands, etc., and changing their garments worn in contact with the sick; lastly, to forbid public funerals for those dying of scarlet-fever. He further says, "Where I have succeeded in having these measures carried out, I have never known the disease to spread further. Beyond thorough ventilation of apartments, and disinfection as above recommended, only such measures as are calculated to promote health and bodily vigor will be of any service to prevent the well from contracting the disease when exposed to it. The use of belladonna, camphor, etc., as preventives, I believe to be utterly valueless, except, perchance, for the *mental* effect upon those having exaggerated fears of the disease. Aside from the cases usually cited in text-books, a case of interest was reported in the London *Lancet* of April 11, 1868, I believe. A domestic servant died of scarlet-fever of very malignant type, after which the doctor gave directions for the most vigilant care in purifying the room and its contents, bedding, clothing, etc.; all which directions were strictly carried out, except with regard to the blankets, which, as the young and newly married mistress objected to the conversion of new blankets into old ones by the process of scouring, were put away uncleansed in a wardrobe in a vacant room. 'Fourteen months afterwards, this young housekeeper, expecting her first confinement, whilst providing a temporary bed in her room for the accommodation of her monthly nurse, took these identical blankets from their resting-place as a part of the covering for it. About a fortnight after making this provision, her labor not having come on in the interval, I was requested to visit her. I found her under scarlet-fever of the most severe form. In four days parturition commenced, and she died from exhaustion in half an hour after the birth of her child.'"

James P. Marsh, M.D., Green Island, N.Y., gives the following from his case-book: "Feb. 3, 1887, Miss M., aged eighteen, came down with scarlatina, which ran a moderately severe course. On Feb. 11, 1887, her nephew, aged five years, came down with the disease, which ran a mild course. Through the whole of his aunt's illness, he was constantly in the room with her, from certain circumstances isolation being impossible. At no time after the beginning of her illness was he out of doors, hence there was no other source of exposure." Dr. Marsh refers to the following articles: 'Practical Considerations Regarding the Acute Infectious Fevers, especially Scarlet-Fever' (*Gaillard's Monthly*, vol. xi. p. 427), 'The Source of Infection and Limits as to the Time of Infection of Scarlet-Fever and Measles' (*New York Medical Record*, vol. xxvii. p. 612), 'Duration of Contagiousness after Scarlet-Fever' (*Transactions of the New York State Medical Association*, vol. i. p. 73), 'Duration of the Infectious Period of Scarlatina' (*New York Medical Journal*, vol. xiv. p. 278).

A. Vanderveer, M.D., Albany, N.Y., reports that healthy children carried in a carriage that had the day before contained cases of scarlet-fever, sickened with the disease in due time.

Winslow Anderson, M.D., San Francisco, Cal., writes that on several occasions, when his patients have been visited while suffering with scarlet-fever, the visitors have carried the disease several miles, and communicated it to children.

Thomas F. Wood, M.D., Wilmington, Del., says, "My children played in a room where some clothing was being quarantined because of a suspicious eruptive disease which was too light to be called scarlet-fever. The boy who came first in contact with the clothes was seized, and two others took it from him."

A. R. Hopkins, M.D., Buffalo, N.Y., relates an instance in which a child, ill with the disease, sent a book from its bed to a neighbor's, the only direct communication between the houses or families. The disease followed the book in five days. In another instance a stuffed chair from a nursery where the disease had been present six months before, was sent by express to a house miles away, where no fever was, or had been, in years. The disease followed the chair in less than two weeks.

Samuel B. Ward, M.D., Albany, N.Y., says, "Many cases have occurred in my practice where one child in a family would catch the disease from some known exposure, outside the house, and within a week other children in the house would take it from the one first affected. N. W., aged eight, was taken with the fever. Three or four days later, A. W., her sister, aged six, took it, the two having slept together before the first was taken ill. The baby, aged two years, was promptly isolated, and escaped for six weeks. Through the carelessness of a nurse he then one day ran down stairs — or rather crept down — into his sick sister's room, came down three days afterwards with the disease, and died of it. E. K. aged five, and L. K. aged three, were attacked at nearly the same time with scarlet-fever. The baby, aged eighteen months, was spending the day with a friend when the discovery was made, and did not return home for two months. In the mean time the other two children recovered. The utmost care was taken with the disinfection of the house by burning large quantities of sulphur with all openings closed, scrubbing the wood-work and floors with bichloride of mercury, leaving all windows open for twenty-four hours after fumigation, washing all bedding and clothing in carbolic acid, etc. After the house was thoroughly warmed again, — it was in winter, — the baby was brought home, took sick with the fever within a week, and died of it. Could the wall-paper have retained the contagion? It was thoroughly swept down, but not removed."

Dr. Ward encloses to us a letter which he has received, and which sufficiently explains itself: "The case you refer to, in your note to me, was that of my daughter, who, in the summer of 1874, after a sojourn of seven weeks at the cottage occupied by my family, and while still there, broke out with scarlet-fever. She had not been away from the place from her arrival there up to the time she was attacked. There was no other case of scarlet-fever at the hotel, or in the cottages connected with it, during that summer. It seems that the family — friends of ours — occupying the cottage contiguous to the one occupied by my family, had, during the winter and spring months just preceding, suffered severely with *scarlatina maligna*, losing one child from among those attacked. In the month of August, about the middle of the month, it is usual to experience at this place a cold storm, generally of three days' duration, when heavy winter clothes are necessary to comfort. Our friends in the cottage contiguous, being habitués of the place, like ourselves, were well provided in this respect, and, during the prevalence of the storm of that year, clad themselves and their children in their winter garments. Dr. Budd, professor in the Medical School of the University of New York City, now deceased, who attended my daughter, had no doubt that my daughter contracted the disease from absorbing the germs quiescent in the woollen winter garments of the children of our friends, with whom my daughter was a constant playmate. The disease in her case, however, though well defined and the eruption profuse, proved a light one, I being able to bring her home on the eleventh day from the first appearance of the disease, without any unfavorable resulting consequences. During the continuance of the illness, every window in my cottage was kept open, save those in my daughter's room, both night and day; the door of her room remaining likewise open, thus admitting freely the sea-wind, whether violent or mild. At one period of her illness there was an incursion of mosquitoes, continuing for several days, so dense that lamps were not lit, and guests moved around or sat about with handkerchiefs upon their heads; the curious fact of which circumstance, however, was the fact that

though every room in my cottage was thick with mosquitoes, excepting that of my daughter, there were only two of these insects at any time observed in her room during the whole period of her illness."

W. W. Johnston, M.D., Washington, D.C., says, "In my own family one case of scarlet-fever occurred: other children escaped. In another family of eight children, isolation and disinfection prevented the spread of the disease, but such instances are numerous."

Charles W. Covernton, M.D., Toronto, Can., ex-president Provincial Board of Health, and Peter H. Bryce, M.D., Toronto, Can., secretary of the Provincial Board of Health, relate an instance where each succeeding member of the family took it at intervals of three or four days. At the period when desquamation of the first was beginning, a younger took a mild form of the disease. A few days afterwards conjunctivitis of both eyes appeared, ending rapidly in the destruction of sight. The disease afterward extended to the middle, with perforation, of tympanum, etc. Thereafter the disease attacked the knee and elbow joints, with intense suppuration and inflammation, ending in their destruction. The child died on the twelfth day. There were some four or five children in all. In the family of one of these physicians, a Cambridge student had a book which he was studying at the time of the seizure with scarlatina. After his death, said book, with others that had been open in the sick-chamber, were packed up and sent to the latter's family in London, where they were placed in a garret. Ten years after, a younger brother at Cambridge sent for these works. Shortly after receiving them, he took scarlatina and died. No other exposure to the disease was known.

Dr. Bryce, in speaking of the methods to be adopted in preventing the spread of the fever, refers to an experience he had five years ago, in the following language: "A child in a family in which there were five children was taken with scarlet-fever. It and its mother were put in an upper room, and the lobby cut off by a curtain antisepticized with a solution of corrosive sublimate. The soiled articles of clothing, etc., were placed in the same solution, and the remnants of food were burned in the fireplace of the room. Seclusion was perfect. At conclusion of desquamation every thing was disinfected. No second case occurred in the family. Dr. Bryce thinks the period of infection is not less than forty days.

EXPLORATION AND TRAVEL.

TRANSVAAL. — The railroad from Delagoa Bay to Transvaal, which was mentioned in *Science*, No. 245, has been opened from Lorenço Marques to the boundary of the Portuguese Possessions. It is somewhat difficult to form a correct idea of the state of affairs in that region, as all news comes from English journals, and as the English are in constant fear of an increase of Boer, German, or Portuguese influence in South Africa. The Boers, of course, make strenuous efforts to open a route to the sea independent of the English, who threaten to swallow up the republics. This aim has been achieved by the new railroad, the greater part of which runs through Transvaal, and is in the hands of the Boers, while the part now opened is in the hands of American capitalists. The opening of this railroad, which was represented by English travellers as improbable, will result in a rapid development of the natural resources of the Transvaal. Although a strong influx of Englishmen into those countries may be expected, it is not probable that they will swamp the Boer element, which has so long resisted the incessant attacks of the English.

ZANZIBAR. — The Sultan of Zanzibar, whose territories have been reduced to a narrow strip of coast-line by recent treaties, has leased his rights on the African coast between Wanga, at the mouth of the Umba, and Vitu, to the British East African Association. As he has made a similar contract with the German East African Association, his rule is practically limited to the islands of Zanzibar and Pemba and several parts of the coast that are of little importance. The part of the coast leased to the British Association includes the whole coast-line between the line of demarcation between German and British influence and the German district of Vitu. It is said that vigorous attempts will be made to open a route from the coast to the Victoria Nyanza.

FARINI AND CHAVANNE. — Dr. Hans Schinz, who made a long and interesting journey in South Africa, undertakes to expose Farini, who claimed to have accomplished a long and hazardous journey to Lake Ngami. He gives convincing proof that Farini, who wrote a large volume on his adventures, never entered the Kalahari, and never came into those remote regions in which he claims to have made important explorations. Several passages in his book had excited the suspicion of scientists; and Schinz gives now, in two letters to *Petermann's Mitteilungen*, conclusive proof that his adventures and discoveries are one great fraud. The work of another African traveller, J. Chavanne, has been justly and severely criticised. Chavanne travelled for some time on the Kongo, and published the results of his observations in a magnificent volume, which is now shown to be largely an audacious plagiarism on other publications on the Kongo, particularly Pechuel-Loesche's important work. Part of Chavanne's own observations are shown to be untrustworthy. Dr. von Danckelmann, who criticised Chavanne, and Schinz, must be congratulated for their courage in exposing these scientific impostors. Nothing should be more rigidly demanded from travellers than truth and a strict distinction between their own observations and those of others. Those infringing these rules cannot be too severely criticised.

THE OBANGI. — Captain van Gèle, who attempted to reach the Welle from the falls of the Itimbiri last summer, but gave up his plan on account of the difficulty of obtaining food at that point, left Leopoldville on Oct. 2 on board the 'En Avant.' He proposed to ascend the Obangi, and thus to ascertain its connection with the Welle. It will be remembered that Grenfell succeeded in ascending the rapids of Zongo, which prevented Van Gèle from exploring the upper part of the river. After having passed these rapids, Van Gèle hopes to find navigable water and to reach the Welle. As it is doubtful whether the Obangi receives a large tributary from the east which may be identical with the Welle, he will carefully examine the left bank of the river, and explore important tributaries which he may discover (*Mouv. géogr.*).

MENTAL SCIENCE.

Re-Action Time for Sensations of Temperature.

IN a recent number of *Pflüger's Archiv* of physiology, Vintschgau and Steinach give a preliminary report of a series of experiments upon the time necessary to perceive a sensation of heat, of cold, or of contact with the skin in various parts of the body. The time necessary for the mere feeling of contact on the middle of the forehead was for Vintschgau .119, and for Steinach .107, of a second. The time of feeling a contact upon the right cheek was .119 and .101 of a second respectively; and similar numbers for the volar and dorsal surface of the left hand are .126, .128, and .133 and .111 of a second. The results of their experiments upon the time it takes to perceive a sensation of cold and of warmth are given in the table below:—

	Cold.		Heat.	
	Vintschgau. (2.2°-4.8° C.)	Steinach. (2°-2.8° C.)	Vintschgau. (48°-49° C.)	Steinach. (45°-49° C.)
Right temple	.160	.116	.166	.132
Left temple	.170	.124	.185	.138
Middle of forehead	.143	.116	.144	.128
Right cheek	.143	.114	.154	.117
Left cheek	.151	.116	.158	.146
Volar surface of hand.				
At middle joint of finger	.186	.152	.205	.173
Near the ulna	.206	.186	.208	.206
On ball of thumb	.185	.194	.251	.175
Dorsal surface of hand				
Near the ulnar side	.208	.179	.246	.199
Near radial side	.204	.170	.233	.196

The conclusions that these tables enable us to draw are, that we feel a sensation of cold more quickly than one of heat, though the difference is slight; again, that we re-act more quickly to sensations of contact than to those of temperature. If the stimulation be applied to the same spot repeatedly and at short intervals, the time is in general lengthened. This was found to be true for the forehead and cheek, for sensations of cold, after a very few minutes. The same is true for the forehead with the stimulation by heat; but on the cheek after fifteen minutes, with the time taken each minute, there was no such lengthening of the time. More details regarding the method of obtaining these results will be given in a future paper.

The same topic has also been investigated by Dr. Goldscheider (*Archiv für Anat. und Phys.*, 1887, v.). His method was to approach a metal ball to the skin, thus breaking an electric connection and re-acting by a simple movement of the jaw. To get reliable results, he chose parts with a thin epidermis, and used an intense stimulus. For cold, the ball was at a temperature of 15° C.; and for warmth, at a temperature of 50°C. In all, over two thousand observations were made. The average of all these times was, for cold, on the face, near edge of the eyelid, .135, on upper arm .150, on the abdominal surface .226, and on the inner surface of the thigh .255, of a second. Corresponding times for the perception of warmth on the four places were .190, .270, .620, and .790 of a second. Warmth is thus much more slowly perceived than cold, and the more so the farther from the brain the part of the body tested, the difference amounting in the lower limbs to nearly half a second. It should be said that care was taken to choose parts of equal sensibility in the several regions of the body. If the stimulation is only moderately strong, and especially if the stimulation is weak, the re-action time is much lengthened. For example: a moderately warm stimulus on the arm takes .46 to .54 of a second to be re-acted upon, and, if the stimulus is weak, it takes .90 of a second to 1.1 seconds. That this lengthening of the time is really an effect of the intensity of the stimulus, is shown by the fact that it occurs in weak stimulation of the most sensitive areas, and not only in strong stimulation of insensitive areas, as would be the case were the lengthening due to the slow radiation through the epidermis. These facts are all in good accord with former investigations of the topic. The explanation of this difference between the re-action time for heat and for cold cannot yet be given. But Dr. Goldscheider is not ready to ascribe it to the fact that the one sensation passes up the white columns of the cord and the other through the gray matter. The results of the two investigations agree fairly well on the time for the perception of cold, but the latter gives much higher values for the re-action time to a warm stimulus.

VISUAL UNITS IN THE RETINA. — In viewing a series of uniformly scattered dots, we will at a certain distance be able to recognize them as dots; but if the object be further removed, they will fuse into a more or less uniform surface. By testing back and forth, one can quite accurately determine the distance from the eye at which the dots are just visible as single dots, and, if we measure the distance between the dots, it is possible to calculate how large a surface on the retina is necessary to impress us with the vision of a separate dot. Such a surface would be a visual unit, and the point of importance is to find what anatomical basis there is for this physiological unit. In 1881 Carl Du Bois-Reymond measured the size of these visual units in the fovea, or yellow spot of the retina, and found that such a unit was exactly the size of a cone at this point. He did not use dots, but rays of light shining through holes in a screen. This makes it extremely probable that a cone is the anatomical unit of vision. Dr. Wertheim (*Graefe's Archiv*, 1887) has continued these determinations for the lateral parts of the retina, where the vision is less fine, and where it is in general known that the number of cones is fewer. In tracing the decrease in the number of visual units to a certain area as we go upwards from the centre of the fovea, he gets a curve, showing at first a marked decrease, then a short period of almost no change, and then a long period of slow, regular decrease. If we ask, How does this harmonize with the anatomy? the answer cannot be as definite as we would wish. The part of the curve showing a marked decrease corresponds to the outer parts of the yellow spot; and the ratio between the number of visual units at the edge of this, compared to the number in an equal surface of the centre, is as one to two or three, while the ratio of the number of the cones in the two places is about as one to three or four. The next period of the curve cannot be thus compared, because the size of the yellow spot is differently determined by different observers. With regard to the lateral portions of the retina, it can be said that the largeness of the visual units makes it necessary that the cones be separated, and this the anatomy bears out. The general conclusion is, then, that the cones are very probably the anatomical basis for the visual units, and that the rods (that become more numerous as we recede from the centre of the fovea) cannot convey the sensation of a single objective point.

THE PSYCHOLOGY OF JOKING. — Dr. Hughlings-Jackson publishes some interesting remarks on this topic in the *Lancet* of Oct. 27. He regards punning as the lowest stage of the evolution of humor, but even in the pun he sees a material for the study of normal mentation. In a pun we have two ideas called to the mind at once, — a double vision, as it were; and, as all thought is the comparison of relations, this is simply a caricature of the normal process of thought. Again: the world owes a great debt to the first punster, because he began the 'play' of the mind (in the same sense as art is founded on the play-instinct), and so detached himself from the grossly useful, and showed a surplus energy capable of developing into the highest traits of mankind. To lack a sense of humor is a bad thing. "The man who has no sense of humor, who takes things to be literally as distinct as they superficially appear, does not see fundamental similarities in the midst of great superficial differences, overlooks the transitions between great contrasts. I do not mean *because* he has no sense of humor, but because he has not the surplus intellect which sense of humor implies." Again: "I think that observation confirms what *a priori* seems likely, — that *pari passu* with the evolution of the sentiment of jocosity (playing at unreality) is the evolution of power of realistic scientific conception, — from sense of the merely ridiculous with parallel realistic conception of simple things, up to sense of humor with parallel realistic conception of complex things." Dr. Jackson then looks upon punning as a 'mental diplopia' in which there is a double mental vision, but not of the kind conducive to useful ends. It is something like the thought in dreams. He sums up his view in these words: "The process of all thought is double, in degrees from a stereoscopic unity of subject and object to manifest diplopia (two objective states in one subject). The process of all thought is tracing relations of resemblance and difference, from simplest perception — to say what a thing is, is to say what it is like and unlike — up to most complex abstract reasoning. The formula of the caricature of the normal process of thought is the 'pretence' of some resemblance between things vastly different, from punning, where the pretended resemblances and real differences are of a simple order, up to humor, where both are highly compound. We have the 'play' of mind in three degrees of evolution, three stages of increasingly complex incongruousnesses."

BOOK-REVIEWS.

Geology and Mining Industry of Leadville, Colorado. With Atlas. By SAMUEL FRANKLIN EMMONS. (U.S. Geol. Surv., Monograph XII.) Washington, Government. 4°.

THE magnificent volume in which the geology of the Mosquito Range, and more particularly that of the environment of Leadville, Col., and its mining industry, is described, contains the results of investigations begun in 1879, at the instance of Clarence King, first director of the United States Geological Survey, and continued until May, 1881. Abstracts of the results of these investigations have been published in the 'Second Annual Report of the Director of the Survey,' but it is only now that the full work and the magnificent atlas have been issued. We will cull only a few points from this great work which are of general interest. The first part of the book deals with geology. A brief history of the discovery and growth of the Leadville region is given. Emmons demonstrates that the paleozoic and mesozoic strata lie unconformably on the Archæan, and, what is of the greatest importance, that the formation which is immediately adjacent to the Archæan varies from

place to place. At one point triassic beds, sloping away at varying angles from the flanks of the mountain, rest directly upon the Archæan beds; at another point, the lower beds of the cretaceous; at still another, and this more rarely, the carboniferous limestones are exposed, resting against the Archæan; while above them, always conformable, are found the triassic, Jurassic, and cretaceous formations, as one follows the section in an ascending geological sense. These facts make it evident that these beds have not been folded into a long anticlinal fold, the crest of which was subsequently planed off by erosion, but that the exposed Archæan parts represent an ancient continent or island along whose shores the younger beds were deposited. The lithological character of the series confirms this view, as they bear internal evidence of being a shore deposit. The Colorado Range is the most extensive of these ancient land-masses. Originally the western boundary of the Park area consisted of two or more masses, forming a general line of elevation parallel to the Colorado Range. Through the south-eastern portion of this area, and parallel with its longer axis, runs the valley of the Upper Arkansas River, which, however, during paleozoic and mesozoic times, did not exist.

The Mosquito Range was not formed until the great dynamic movement in the Rocky Mountain region at the close of the cretaceous. Enormous masses of eruptive rocks are found in this region crossing the sedimentary strata to greater or less elevations, and then spreading out in immense sheets along the planes of division between the different strata. From the fact that these interbedded sheets of eruptive rocks are found practically conformable with their bounding strata, and, like them, folded into sharp folds and cut off by faults, Emmons concludes that the eruptive activity preceded the uplift of the Mosquito Range. The latter was effected by a pushing-together from the east and from the west, a secondary movement acting in a north-and-south direction. The Archæan masses, between which the conformable series was deposited, the resistance of which caused the crumpling of the beds, must have participated in the folding.

A special chapter is devoted to the discussion of the geological phenomena and theoretical questions. The most important of these are the discussion on the folds and faults, and a comparison of the monoclinal folds and the great faults of the Great Basin with those of the Rocky Mountains. Emmons believes that the former are folds similar to those of the eastern mountainous region. He considers them true plications, and believes, that, could the structure beneath the valley be seen, the missing faulted-down members of the fold would be found. His principal objection against the reading of the geological structure of the Great Basin accepted by many scientists, that it is a region of faulted blocks uplifted in different directions, and practically without plication, is, that this theory would involve the actual annihilation of considerable wedge-shaped segments of stratified beds by the simple action of faulting. His theories of the origin of mountain-ranges are in accordance with Suess's theories. He denies the existence of an uplifting force, but considers the faults as caused by contraction and consequent sinking, while the folding is caused by tangential pushing and crumpling of superficial strata of the earth's crust. Another object which he discusses fully is the origin of dolomites and serpentine, the origin of the intrusive masses, and the improbability of sedimentary rocks being absorbed by eruptive masses.

The second part of the volume deals with the mining industry, with the origin of the metal deposits, and the methods of smelting. The atlas contains, besides numerous sections, a reprint of the Hayden map of Central Colorado, and a topographical map of the Mosquito Range drawn so that the light falls from the north-west and at an angle of 45° upon the mountains, by which method the topographical features appear very clear and distinct.

An Inquiry into Socialism. By THOMAS KIRKUP. New York, Longmans. 12°.

THE author of this book declares himself a socialist, but he means by socialism something quite different from what usually passes by that name. He does not favor communism, nor State socialism, nor an equal division of property; and he condemns all anarchical and revolutionary methods. He would extend the powers of government to a certain extent, especially in the municipalities. But he means by socialism chiefly what other folks call co-operation, — the ownership of the means of production by voluntary associations of laborers. He remarks, as many others have done before him, that the main defect in our present industrial organization is the divorce of the laborers from land and capital. But as the individual ownership of land and capital is becoming impossible, the only way out of the difficulty is by the joint ownership of both by associations of laborers. Yet he does not propose, like most of those who call themselves socialists, to take the property away from those who now possess it without giving them compensation: he proposes to pay for it. Moreover, he does not favor doing it by the action of the State, but by the gradual extension of voluntary co-operation. In short, he lays down as the cardinal principle of socialism, that, "whereas industry is at present carried on by private capitalists served by wage-labor, it must in the future be conducted by associated or co-operating workmen jointly owning the means of production" (p. 94).

Now, it is clear that such a system as this is very different from what is commonly called socialism, and we believe that most of those that style themselves socialists would repudiate it. Certainly they show at present no inclination toward voluntary co-operation; for if they really favored it, as Mr. Kirkup does, they would set about organizing co-operative societies. We admit, however, that Mr. Kirkup's socialism is a great improvement on that which is commonly so called; but then it does not differ essentially from what economists have always advocated under the name of 'co-operation.' Most economists of the orthodox school would disagree with Mr. Kirkup in regard to extending the functions of government; but otherwise they would have little to say against the system he advocates as an ideal for the future. He paints the evils of the present system, with its millionnaires and its beggars, in a vivid light, and with too little attention to its better features; yet he admits that skilled laborers, at least, are better off now than formerly. With regard to the prospects of the system he advocates, he does not speak in the most sanguine terms; and he clearly recognizes the difficulties in the way of its establishment. Indeed, he expressly says, that, "without a great moral advance, socialism may be regarded as impracticable" (p. 159), — an opinion in which most advocates of co-operation will be likely to agree. Mr. Kirkup's style is fairly good, and he has made an interesting book; but we very much doubt if it will meet with much approval among the mass of those who call themselves socialists; while at the same time his use of the term 'socialism' to designate the system he advocates is liable to raise a prejudice against it in the minds of others.

NOTES AND NEWS.

M. MOISSAN describes, in the *Annales de Chimie et Physique*, his long-continued experiments for isolating fluorine. While all former attempts to reach this result failed, M. Moissan, after many failures and disappointments, succeeded in his endeavors by electrolyzing anhydrous hydrofluoric acid in which the double fluoride of potassium and hydrogen was dissolved. *Nature*, in describing Moissan's experiments, gives a *résumé* of the remarkable qualities of fluorine as observed by Moissan. Sulphur, brought near the orifice, at once melted and inflamed; selenium behaved in like manner; as did also tellurium, with incandescence, forming fumes, and becoming coated with a solid fluoride. Phosphorus at once took fire, forming tri-, penta-, and oxyfluorides. Powdered arsenic and antimony combined with incandescence, the former yielding drops of AsF_3. A fragment of iodine placed in the gas combined, with production of a pale blue flame; in an atmosphere of iodine vapor, fluorine itself burned with a similar flame. Vapor of bromine lost its color, and the combination was sometimes accompanied by detonation. Cold crystalline silicon at once became incandescent, and burned with great brilliancy, sometimes with scintillations. On closing the little tubes containing it with the thumb, and opening under water, the silicon tetrafluoride formed was absorbed and decomposed, with precipitation of silica. Any undecomposed silicon was found to have been fused. Debray's adamantine boron also burned in the gas, becoming incandescent, and giving off fumes. Fluorine has a most extreme affinity for hydrogen: they combine in the dark, with explosion. In one of the

experiments the electrolysis was allowed to continue several hours, so that eventually the small quantity of undecomposed acid remaining in the **U**-tube was insufficient to keep the two gases apart: the experimenters were consequently suddenly startled by a violent detonation. The hydrogen and fluorine had combined in the dark at the low temperature of — 23°. The same detonation was afterwards brought about on a smaller scale by reversing the current. On bringing the wide-mouthed delivery-tube of a hydrogen-generator near the orifice, the detonation at once occurred, and the hydrogen inflamed. Metals are all attacked with more or less energy by fluorine, forming fluorides. Cold sodium and potassium were at once rendered incandescent. Calcium, magnesium, and aluminium acted similarly, in a more modified manner, becoming incandescent when slightly warmed. Powdered iron and manganese, on gently warming, burned with bright scintillations. Lead was attacked in the cold, and tin at a slightly elevated temperature. Mercury, as suspected, entirely absorbed the gas, forming yellow protofluoride. Silver, at a gentle heat, became coated with a beautiful satin-like fluoride, soluble, unlike the chloride, in water. Gold and platinum at 300°–400° became coated with their respective fluorides, which were decomposed again at a red heat, with evolution of free fluorine. Perhaps the strongest evidence of the intense chemical activity of fluorine is exhibited in its action upon cold potassium chloride: the chlorine was at once expelled, filling the air with its disagreeable odor, and was identified by the usual chemical tests. Chlorine was also expelled from its combination with carbon in carbon tetrachloride. All organic compounds are violently attacked by fluorine; a piece of cork at once carbonized and inflamed; alcohol, ether, benzene, and turpentine took fire immediately in contact with it. Glass, as might have been expected, is at once corroded by fluorine: some very delicate experiments were carried out with perfectly dried glass, with the same result. Many other re-actions, all interesting and all showing the immense energy with which the atoms of fluorine are endowed, were performed, but one especially ought to be noticed; viz., the action of fluorine upon water. It is a singular fact, that, whenever oxygen is liberated in the cold, there is a great tendency to form ozone: hence, when fluorine is attempted to be collected over water, the gas collected is not fluorine, but ozonized oxygen; water is decomposed by the fluorine, forming hydrofluoric acid, while the oxygen is set free, and a considerable quantity of it is converted into the more condensed form of ozone.

— A new journal for promoting the teaching of physics and chemistry is being published in Berlin (*Zeitschrift für den physicalischen und chemischen Unterricht*, Springer). It is edited by Dr. F. Poske. In an introductory note, the editor emphasizes the educational value of the teaching of physics. He says that it must show how the knowledge of physics originates, — historically and logically, — and that by doing so it is as valuable a means of education as any other science. The first number contains a paper by the eminent physicist and philosopher, E. Mach, on the teaching of the physics of heat, and another by M. Koppe on Foucault's pendulum experiment. There are numerous descriptions and illustrations of simple apparatus for demonstrating physical experiments in school.

— The third annual meeting of the Indiana Academy of Science was held at Indianapolis, Dec. 28 and 29. The following is a list of the papers read: D. W. Dennis, 'The East-West Diameter of the Silurian Island about Cincinnati;' C. R. Dryer, 'The Kames of Allen County, Ind.;' J. T. Scovell, 'Erosion in Indiana;' D. A. Owen, 'A Geological Section of Johnson County, Ind.;' D. W. Dennis, 'The Transition of *Orthis occidentalis*, Hall, into *Orthis sinuata*, Hall;' O. P. Hay, 'Notes on Some Fossil Bones found in Indiana;' O. P. Jenkins and W. V. Brown, 'Location of Eel River Falls;' J. C. Branner, 'A Sketch of the Geology of Arkansas' and 'The meanderings of the Arkansas River below Little Rock;' J. U. Nef, 'On Carboxylated Derivatives of Benzoquinone;' W. A. Noyes, '*Beta* para Nitro-toluic Acid;' J. U. Nef, 'On Chloranil;' J. L. Campbell, 'The Reversal of the Electric Current in the Holtz Induction Machine;' C. A. Waldo, 'A Method of Determining the Epicentrum of an Earthquake;' B. W. Evermann, 'The Fishes of Carroll County, Ind.;' W. P. Shannon, 'A List of the Fishes of Decatur County, Ind.;' D. S. Jordan, 'The Isthmus of Panama as a Barrier to Marine Fauna;' O. P. Jenkins, 'Notes on Some Southern Indiana Fishes;' D. S. Jordan, 'Blind Fishes and Natural Selection;' F. M. Webster, 'An Unusual Appearance of *Apatura celtis* along the St. Francis River in Arkansas;' J. S. Kingsley, 'The Origin of Anthropods;' G. G. Hubbard, 'List of Butterflies of Jefferson County, Ind.;' W. P. Shannon, 'List of Butterflies of Decatur County, Ind.;' F. M. Webster, 'Drouth, and its Effect upon Insect Increase and Decrease,' 'Distribution of Some Species of Injurious Insects, throughout Indiana, during the Season of 1887,' and 'The overflow of the Mississippi River, and its Effect upon the Species of *Simulium* (Buffalo Gnats) infesting the Smaller Inland Streams of the Adjacent County;' Amos W. Butler, 'Some Rare Indiana Birds;' Maurice Thompson, 'The Secondary Functions of the Hyoid Cornua in *Picus* and *Colaptes*;' Amos W. Butler, 'Suggestions concerning a Law for the Protection of Birds;' D. S. Jordan, 'The Origin of Genera;' C. W. Hargitt, 'Some Curious Monstrosities in Egg-Formation;' W. S. Windle, 'The Skull of *Necturus lateralis*;' J. M. Coulter, 'Evolution in the Vegetable Kingdom' (presidential address); C. W. Hargitt, 'Notes on *Scaphiopus holbrookii*;' O. P. Hay, 'Observations on the *Amphiuma*;' B. W. Evermann, 'The Occurrence of the Star-nosed Mole in Indiana;' A. W. Butler, 'Notes on Some Indiana Reptiles and Amphibians;' O. P. Hay, 'Some Additions to the List of Indiana Reptiles;' Lillie J. Martin, 'A Chemical Study of *Juglans nigra*,' and 'The Value of Organized Work in Plant-Chemistry;' O. M. Meyncke, 'The Late Drouth and its Effect on Vegetation;' Stanley Coulter, 'Histology of the Foliage Leaf of *Taxodium distichum*;' John M. Coulter, 'Stomata of *Tillandsia usneoides*;' G. G. Hubbard, 'Additions to the Flora of Indiana;' J. N. Rose, 'Characters in *Umbelliferæ*;' O. M. Meyncke, 'Companion Plants;' Walter H. Evans, 'Lichens of Indiana;' J. C. Arthur, 'Life-History of the Plum-Leaf Fungus;' O. M. Meyncke, 'Notes on the White-spored Agarics of Franklin County, Ind.;' T. B. Redding, 'Man an Evolution: Biological Proofs.'

— A meeting for the purpose of organizing the American Folk-Lore Society was held at University Hall, Harvard University, Cambridge, Mass., on Wednesday, Jan. 4. Rules for the government of the society were enacted, of which the first declares that "the American Folk-Lore Society has for its object the study of folk-lore in general, and in particular the collection and publication of the folk-lore of North America." The rules further provide that the society shall consist of members who subscribe an annual fee of three dollars; that each member shall be entitled to a copy of the journal to be issued by the society; that an annual meeting shall be held; and that the affairs of the society shall be conducted by a president and a council of fourteen members, to be elected annually. Prof. E. J. Child of Harvard University was elected president.

— The *Railway Review* of Jan. 7 says that on Dec. 31, 1885, there were 10,746 miles of railways in operation in South America, of which 4,378 were situated in Brazil. We have compiled the statements given in the *Annuario do Imperial Observatorio do Rio de Janeiro* of 1887. It appears that on Dec. 31, 1886, approximately 4,820 miles of roads were in operation, while 2,530 miles were being constructed and surveyed. The statements given in the *Annuario* are not sufficiently clear to give exact figures for the lines. The value of the information given in the annual is enhanced by tables giving the elevations of the stations. According to the *Annuario* of 1886, 4,607,213 miles of telegraph-lines were in operation, of which 1,325,894 miles are in the Province Rio Grande do Sul.

— A course of eight lectures on subjects of general interest is to be given by leading scientific men in behalf of the Marine Biological Laboratory, under the auspices of the Boston Society of Natural History. The Marine Biological Laboratory is to be a permanent station on the New England coast, where suitable opportunities and conveniences may be had for teachers, professional naturalists, and others, to collect and study the animals and plants of the sea. The project has the support of the naturalists of the country and of many liberal citizens, who have already contributed several thousand dollars toward the funds needed. The receipts from the lectures will be applied to increase the funds. If a sufficient sum is

obtained now, the laboratory will be opened next summer. The following is a list of the lecturers and their subjects: Jan. 18, Prof. W. H. Niles of the Massachusetts Institute of Technology, 'Mountain Sculpture;' Jan. 25, Maj. J. W. Powell, director of the United States Geological Survey, 'Savagery, Barbarism, and Civilization;' Feb. 1, Prof. H. N. Martin of the Johns Hopkins University, 'A Hen's Egg;' Feb. 8, Prof. George L. Goodale of Harvard College, 'Seeds;' Feb. 15, Prof. F. W. Putnam, director of the Peabody Museum of American Archæology and Ethnology, at Cambridge, 'The Serpent Mound and the Ancient People of the Ohio Valley;' Feb. 22, Prof. Alpheus Hyatt, curator of the Boston Society of Natural History, 'A practical Example of the Evidence for Evolution;' Feb. 29, Dr. Henry P. Bowditch, dean of the Harvard Medical School (subject to be announced); March 7, Prof. Edward S. Morse, director of the Peabody Academy of Science, Salem, 'Reptilian Affinities of Mammals.'

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Twenty copies of the number containing his communication will be furnished free to any correspondent on request.

The editor will be glad to publish any queries consonant with the character of the journal.

The Trinity Formation of Arkansas, Indian Territory, and Texas.

DURING the past field-season the writer has had an opportunity to study the small mesozoic area in the south-west corner of the State of Arkansas and south-eastern Indian Territory, which is the north-eastern termination of the great area so well developed to the southward in Texas. By courtesy of Dr. John C. Branner, State geologist, I am permitted to publish the following note in advance of the more detailed official report which will soon be published by him.

In previous papers (*American Naturalist*, Feb. 1887; *American Journal of Science*, April and October, 1887) I have shown that the mesozoic strata of the Texas region, instead of belonging to the uppermost cretaceous as had been previously supposed, really embraced a large series of lower cretaceous and perhaps Jurassic beds. To the last-named period I intimated that the strata in Parker County, Tex., provisionally termed in my section the 'Dinosaur Sands,' would probably be found to be related. The studies of the past season in Arkansas have shown that these strata exhibit great uniformity of deposition along the paleozoic and mesozoic parting from south of the Brazos River in Texas, to the Little Missouri River near Antoine, Pike County, Ark., a distance of over three hundred miles, and that they rest directly upon the highly disturbed carboniferous rocks. In Texas the areal extent of this formation coincides with the eastern half of the Upper Cross Timbers, and in Arkansas it extends from the point above mentioned westward to beyond Ultima Thule. Its width, except for a few miles on each side of Red River, never exceeds a few miles. The formation consists of alternations of fine, closely packed white sands and red and blue gypsiferous marls, with occasional alternations of thin but extensive, fissile, arenaceous, and crystalline limestones, highly fossiliferous, often wave-marked, and seldom more than ten inches in thickness. Extensive strata of pure saccharoidal gypsum also occur in places, and the formation is the source of the salines and salt licks throughout its extent, and probably also of the 'brackishness' of the rivers which intersect it.

This formation is clearly distinguished from the overlying cretaceous (which deposits are later and later as we proceed eastward along the contact) and the underlying carboniferous. West of Weatherford the basal Comanche series may be seen resting directly upon it, while, at the point of its disappearance under the newer strata in Arkansas, it is directly covered by the uppermost cretaceous of Hilgard's Mississippi section.

The fauna of this formation is littoral and of great uniformity throughout its extent, and, upon hasty observation, conveys an impression that it is later than it really is. It consists of characteristic molluscan species which are hardly distinguishable from certain characteristic European forms specially indicative of the Upper Jurassic and Wealdan. I hope to give more detail concerning these fossils in a special paper hereafter. In Texas I found what are at present supposed to be dinosaurian remains; and occasional vegetal remains are met with.

To the continuous formation the name of 'Trinity' is applied, from the rivers of that name which arise in it. This includes the strata which I termed 'Dinosaur Sands' in my Texas section.

The discovery of these trans-Mississippi beds of Jurassic affinities is of importance, in that it indicates a close relation and possible continuity between the pre-cretaceous mesozoics of Colorado and the Texas Pan-handle, and the Tuscaloosa and Potomac beds of the cis-Mississippi region. ROB'T T. HILL.

U. S. Geol. Surv., Washington, D.C., Jan. 6.

Children's Development.

RECENTLY I became interested in the vocabulary of my boy, thirty months old, and for one day noted all words used by him, except proper names. No effort was made to exhaust the child's stock of words by questioning. He used three hundred and fifty-two words, of which fifty-four per cent were nouns, eighteen per cent verbs, and eleven adjectives. It is probable that the child's entire vocabulary of dictionary words includes four hundred or more. G.

Washington, D.C., Jan. 4.

Is there a Venomous Lizard (Heloderma)?

THIS animal has been an object of considerable interest to naturalists because of the question whether or not it presents the anomaly of a venomous lizard. Just before leaving the United States, last September, I had under my care about twenty so-called 'venomous lizards' of various ages and sizes; and, as I believe the biography of this animal has been but slightly touched on, a few observations in regard to them may not be out of place.

They varied in length from 19 to 49.5 centimetres. The larger ones, say above 43 centimetres, were all females. Their colors ranged from almost a brick-red to pale pinkish white, with markings from black to vandyke brown, which showed no regularity in details, appearing as if each lizard had been the subject of some Chinese artist who aimed only at the general effect. They all came to my father's establishment, in Rochester, by express; and the shaking-up and lack of freedom that they had undergone served to make them very irritable. When first liberated from their confining boxes, their first desire was to get hold of the nearest person, and, although usually very sluggish, they would then move with surprising agility, turning end for end, and making short dashes hither and thither with great swiftness. When one succeeded in fastening its teeth in my clothes, it held on with the tenacity of a bull-dog, occasionally giving a vicious shake to its head, as if trying to tear away a piece of the cloth. Nor was this pugnacity confined alone to the time of their arrival, but continued in lesser degrees during the entire time that I had them under observation. Once I saw a pitched battle between two. One had its teeth firmly fixed in the throat of the other, who, in turn, had a leg of the first in its jaws. Together they rolled and twisted over the floor, neither relaxing its hold for a period of fifteen minutes. Blood was drawn on both sides, yet neither afterwards appeared the worse for the conflict. I then tried two of them on a hen, to ascertain if they would prove poisonous to her. Having first shaved the thigh of the hen, so that the feathers might not interfere wth the entrance of any poison, I induced one of the lizards to take hold. This it readily did, and retained its grip for five minutes, occasionally shaking its head in a savage manner. During the operation the hen appeared quite impassive, and, although not tied, made no attempts to escape, evidently charmed by the lizard. A little blood was drawn, showing that the flesh had been thoroughly pierced. For perhaps a half-hour afterwards the hen appeared a trifle stupid, but soon regained its normal condition, and gave no signs at all of poisoning. Two days later I repeated the experiment with another lizard, with a similar lack of results. I then caused one of them to bite the edge of a saucer, and, with a hypodermic syringe, injected the fluid obtained in the breast of a pigeon. No effect. Then, exciting one so that it viciously bit a small piece of wood, I drew a considerable quantity of fluid direct from its mouth, which, injected into the pigeon's breast, produced no results.

However, birds and lizards are bad subjects for experimenting upon with supposed poisons, and do not conclusively prove that they might not be poisonous, or perhaps even fatal, to man. But being very busy at the time, I had no opportunity to carry my experiments further.

The forked tongue continually playing in and out of the mouth like a serpent's, the snake-like hiss, and the bright colors, together with their aggressive disposition, are well calculated to excite the suspicions of the Arizona Indians, who are reputed to greatly fear and thoroughly believe in the extreme venomousness of this reptile.

When intent on going anywhere in particular, their gait changes from a dragging of the body along the ground to that which an alligator assumes under similar circumstances; i.e., the body is carried high on the legs, clear from the ground, and the tail carried rigid and in line with the body.

They showed a peculiar fondness for water. When placed in a large tank with sloping bottom, in one end of which was water, all would spend most of their time lying where it was about an inch deep. This appears strange when recalling the arid character of the plains that they inhabit.

Their rations consisted of raw hen's-eggs, one of which made a full meal for a good-sized individual, which would not appear to care to dine more than once in about four days. These were given whole to the larger ones, which, having gotten the egg fairly in their jaws, experienced no difficulty in breaking the shell. Their mode of eating is by running the tongue into the mass of the egg, drawing it into the mouth, repeating this in a very deliberate manner, and spending from twenty minutes to a half-hour on an egg.

Their ability to climb is considerable; quite out of keeping with their heavy, unwieldy appearance. A tolerably smooth stick, an inch in diameter, standing at an angle of about sixty degrees, is quite easily ascended.

Several of them laid eggs during August and September. These were 53 millimetres long by 26 millimetres in transverse diameter, were perfect ellipsoids, having a finely granulated, soft, tough, translucent skin or shell.

HENRY L. WARD.

Tambaya, D.F., Mex., Dec. 25.

Sections of Fossils.

IN *Science* for Nov. 18, Prof. Joseph F. James, in speaking of the production of sections of *Bryozoa* for microscopic examination, says, "I can quote no higher authority than Mr. Archibald Geikie (*Text-Book of Geology*, pp. 85–88, where elaborate directions are given for making rock sections; Professor Prestwich also considers it 'an expensive and tedious process,' *Geology*, i. p. 43) as to the tediousness of the process." The pertinence of these references immediately vanishes if a person take but the pains to look them up. In both it will be found that the authors have been referring to the making of slides of Plutonic and metamorphic rocks. Of course, any one knows that a limestone in which *Bryozoa* are usually embedded cuts far more readily than crystalline rocks. Now, with a little practice, a man can soon cut from six to ten slides of crystalline rocks in a day; and he can cut six times as many slides of calcareous *Bryozoa* in the same time, as I have often seen done by college students, not by lapidaries. An average of from forty to sixty slides a day certainly cannot be complained of. Of course, no one will deny that the use of the microscope in fine petrographical studies of crystalline rocks has become imperative. We are here referring to *Bryozoa*.

Feeling convinced, from my own study of the writings of these authors, that they had never expressed an opinion of this subject, least of all with special reference to the *Bryozoa*, I sought for further information. Under date of Dec. 10, Prof. Joseph Prestwich writes me, "The question you ask about the *Bryozoa* is quite beyond my knowledge. I have never studied the *Bryozoa*. In fact, there are very few persons in England who have studied them. We lost our great authority in my old friend Mr. George Busk." In a letter dated Dec. 8, Prof. Archibald Geikie writes, "The question you propose to me in your letter is really one to which I do not feel myself competent to give an answer. I haver never given special study to the *Bryozoa*, and I have nowhere ventured to publish an expression of opinion."

The sentence quoted from Professor James's article concludes with the following words: "nor a better one than Dr. Nicholson as to the uncertainty of the results." In my article of Nov. 4, I mentioned Prof. H. A. Nicholson as one of the leading men who first took a decided stand in favor of the prominent use of internal characters as a means of classification. Now, it would not be fair to construe the above sentence as meaning that Professor Nicholson's writings are themselves a manifest example of the viciousness of the methods pursued by the new school. It must mean, therefore, that Professor Nicholson does not believe in the use of these microscopic sections. Since we interpret the spirit of Professor Nicholson's 'Genus Monticulipora' (1881) and 'Tabulate Corals' (1879) so differently, it will certainly be fair to quote his later writings, since they at the same time must contain his more mature views. Thus in the *Annals and Magazine of Natural History*, February, 1884, he writes, "The earlier observers of these fossils, as, for example, Mr. Lonsdale, necessarily founded their names upon macroscopic characters principally, the method of investigation by means of thin sections being of recent origin; and they also gave, as a rule, extremely brief descriptions. Hence it is exceedingly difficult, in many cases, among the monticuliporoids, to be certain as to the precise forms to which the older names should be attached." Then he proceeds to an investigation of both external and internal characteristics, accompanying the same with figures, of which those illustrating internal features alone are of value. In the number for December, 1885, he and Foord discuss the genus *Fistulipora* on the basis of the new light cast upon it by an investigation of the internal structure. Again in May, 1886, they make use of this method when they say, "Having recently had the opportunity of making a microscopical examination of a very extensive series of these forms, we have satisfied ourselves that they cannot be referred to the genus *Chætetes*, Fisher." And they propose the new genus *Rhaphidopora*. The plates 15, 16, and 17, accompanying this article, do not leave any doubt as to the position taken by these authors. The same is true of an article published by Nicholson and Etheridge in the same journal (March, 1886), where indeed they go so far as to separate *Stenopora australis* from *S. ovata*, with which "the specimens in question agreed entirely in external form and in macroscopic characters," solely on the basis of distinct internal features.

I cannot do better to express the opinions which actuate the new school of students than to quote from a letter from Prof. Archibald Geikie: "The common-sense view of such questions seems to me to be this. In dealing with fossils we are precluded in a vast number of cases from appealing to the evidence of internal structure, for it has not been preserved. Hence, if an organism can be satisfactorily determined from external characters, that is the most desirable means of identification, for it is the most generally applicable. If external characters are proved to be insufficient, and even misleading, we must fall back on internal structure when we can get it." Now, the new school believe that external characters often are misleading, where internal characters may more safely be followed. Since any *Bryozoa*, to be determined even according to the old method, must have the minute external structure well shown, and since in these cases the minute internal structure is also usually well preserved, we believe that the new method is eminently practicable. Nobody denies that external characters may be of great additional assistance.

AUG. F. FOERSTE.

Cambridge, Mass., Dec. 29.

Weather-Predicting.

IT has become a well-worn adage that half of the disputes would be avoided if the disputants had a thorough mutual understanding of the terms used by each. In weather predictions and verifications a clear understanding of the meaning of the terms used certainly seems very necessary. If a weather-predictor concludes that a satisfactory definition of a fair day is one on which less than .01 of an inch of rain falls, and a foul day is one on which more than .01 of an inch falls, and makes predictions accordingly, his predictions, when verified by this rule, will give a certain success in proportion to his skill. If, now, some one should object to cloudy days without rain being called fair, and record all cloudy days for which fair weather had been predicted as failures, he would give the predictions a much lower percentage of success

than by the first method. If he should go still further, and object to calling a day foul unless at least .05 of an inch of rain fell, and proceed to verify the above predictions accordingly, the percentage of success would rapidly approach zero. By disregarding this evident truth, Prof. H. A. Hazen has, in his letter on p. 322 of the last volume of *Science*, involved himself apparently in great confusion.

Mr. Rotch and the writer have during the last year published statements showing that local predictions issued from the Blue Hill Observatory for longer periods in advance than those issued by the Signal Service for this vicinity have had a higher percentage of success than the predictions of the latter. Some of these statements were copied in the notes of foreign meteorological journals, and were prominently referred to in an article by Dr. Klein.

In September, 1887, letters were received from Professor Hazen in which he referred to these statements, and said that our supposed higher success was 'all moonshine,' and was entirely due to our methods of verification. Moreover, he said it was unfair to verify predictions made for Massachusetts by the Boston record alone, and proposed that he and the writer should try together predicting for Boston alone. This seemed eminently fair, and the writer agreed to it; but, to make sure that both had a clear understanding of the meaning of the terms to be used, definitions of the terms 'fair weather,' etc., used by the writer in making predictions, published by the Associated Press of southern New England, were sent to Professor Hazen. He materially modified these, and sent the following definitions and rules. The temperature rules are omitted.

PLAN FOR WEATHER AND TEMPERATURE PREDICTIONS AND VERIFICATIONS AT BOSTON AND WASHINGTON (ALL VERIFICATIONS TO DEPEND ON THE OBSERVATIONS [TAKEN TRI-DAILY AT BOSTON]; PREDICTIONS TO BE MADE AT OR BEFORE 2 P.M., TO HOLD FROM MIDNIGHT TO MIDNIGHT).

Prediction: Fair Weather. — Successful: if fair three times; cloudy, fair, clear in any order; and any cloudiness less. Failure: if cloudy twice in any order; cloudy, fair, fair in any order, and any cloudiness above; a drop of rain.

Prediction: Threatening. — Success: if cloudy twice in any order; cloudy, fair, fair and any cloudiness above; rain .01 or less. Failure: if fair three times; cloudy, fair, clear in any order; and any cloudiness less; rain over .01.

Prediction: Rain. — Success: rain at any time over .01. Failure: rain .01 or less and any cloudiness.

Predictions were begun according to these rules, and the writer sent Professor Hazen a prediction during each day in October except on Sundays. Professor Hazen has correctly given these predictions, with the corresponding weather at Boston, on p. 323 of the last volume of *Science*. If any one will take these tables, and carefully verify the predictions in accordance with the above rules, he will find that sixteen of the predictions in Column 1, which represent the Blue Hill predictions, were verified, that is, sixty-four per cent of the whole; while only twelve of No. 2 (Professor Hazen's) were verified, or forty-eight per cent of the whole. This excess of sixteen per cent for Blue Hill apparently did not suit Professor Hazen, and he proceeds to obtain from Professors Russell and Upton other definitions and rules for verifying fair, threatening, and rainy weather; and, finding that these give a higher per cent for No. 2, he omits entirely to give his own rules. The writer likes Professor Upton's scheme better than that of Professor Hazen, only his predictions were not made in accordance with such a scheme. The predictions sent to Professor Hazen were not made to be verified in detail, but only to agree with his rules; and it so happened, that, while the writer was predicting with Professor Hazen, he was also predicting for the Boston papers; and when he predicted in these, "rain followed by fair weather," or *vice versa*, he merely wrote on Professor Hazen's card "rain," because, according to Professor Hazen's rules, any rain of over .01 of an inch was to be accounted success. Hence it is seen to be manifestly unfair to verify them by other rules.

According to the definitions sent to voluntary observers by the Signal Office, a fair day is one on which less than .01 of an inch of rain or snow (melted) fell, while a foul day is one on which .01 of an inch or more fell; and the writer was recently told by one of the predicting officers of the Signal Service that this was virtually the method used in the official verifications.

At Blue Hill this definition has been adopted, and hence the predictions are exactly comparable with those of the Signal Service. For October the Blue Hill predictions thus verified gave a percentage of success of eighty-five, while the Signal Service predictions only gave fifty-eight per cent for this vicinity. In both cases Sundays were omitted. Professor Hazen knew how this percentage was obtained, and yet in his letter to *Science* he writes as if it were a surprising thing that the same predictions should give eighty-five per cent when two things were considered, and only sixty-four per cent when three things were considered, in the verification.

H. HELM CLAYTON.

Blue Hill Observatory, Jan. 4.

American Microscopes.

IN my letter to *Science* (x. No. 252) in regard to American microscopes, I stated that my opinion in regard to them was based upon the examination of those brought to me by students. I hoped thus to avoid the appearance of claiming to have made an exhaustive examination of all forms of American microscopes. I regret that I did not make an express disclaimer.

Dr. Prudden has placed me under obligation by his very courteous letter in *Science* of Dec. 23, which calls attention to Grunow's new stands. Dr. Prudden's surmise that I was unaware of Grunow's recent work is correct. It is with much pleasure that I now learn that he is endeavoring to meet so admirably the demands of professional biologists and the needs of students.

Mr. Edward Bausch considers me unjust, if I do not misinterpret his letter (*Science*, Dec. 23). He appears to me to have overlooked that I wrote only in regard to microscopes suitable for biological, and particularly histological work. I have heard that the elaborate American stands were favorites with amateurs, but in regard to that point I expressed no opinion. I believe, however, that the increased demand for what is known as the continental stand is due to the rapid growth in numbers of those who use the microscope as a professional instrument, and to the extensive introduction of laboratory work in histology as a part of the course of instruction in our colleges and medical schools.

In regard to the Harvard microscope, Mr. Bausch may recollect, that, when he first came to consult me, I then urged upon him the advisability of frankly imitating one of the Zeiss stands. This advice he decided not to follow. At the time of his second visit I think that I again expressed to him the same advice. I also counselled him to make certain essential and some minor alterations. He made all of the latter, none of the former, if my memory is correct. He subsequently sent me a stand and two objectives to test. In reply I wrote the opinion which he has quoted in his letter, and which I see no occasion to alter now, but am compelled to append a remark for my own justification. The remark is, that I have since then examined a number of the Harvard microscopes brought to me by students. The stands have been of fairly good workmanship, but the objectives I have found, by conscientious examination, to be not infrequently of inferior quality, and most decidedly not satisfactory. As far, therefore, as my experience enables me to judge, I still feel disinclined to bestow the commendation upon these special American microscopes which I am ready to give to some of their foreign competitors.

My letter was not intended to impugn the honesty of the American manufacturers of microscopes, and I do not wish to do so at all. I do wish to call attention to the fact that their policy has been to supply instruments, which, however suitable for certain persons, are not as satisfactory for the work of the professional biologist, the medical practitioner, and of students, as are certain of the European microscopes.

It is to be hoped that Professor Ryder's interesting letter will bring about the result he suggests, of having a competent committee take up the consideration of the best attainable microscope. For my own part, I feel much pleased with a German stand of quite new model, which I purchased last summer. After using it a good deal, I have little change to wish for in it. If it should please others equally, it may be considered to represent an advance towards

the ideal anticipated by Professor Ryder. As to the duty on scientific instruments and books, probably the scientific men of the country object unanimously. One of them said to me once, "When I express myself mildly, I call it a disgrace to the country and an outrage on science." *Science* might accomplish a valuable service by collecting and publishing expressions of opinion on this part of the tariff from some of the leading scientific men of the country. Would not a petition to Congress to abolish the duty on scientific instruments and books in foreign languages find many and distinguished signers? CHARLES SEDGWICK MINOT.

Boston, Dec. 23.

Arkansaw and Kansaw.

WHERE can one find a copy of the law fixing the pronunciation of 'Arkansas'?

As I remember the phraseology, it runs thus: "Each *a* shall be sounded as *a* in 'father,'" or, "Each *a* shall have the Italian sound of *a*, as in 'far,' 'father,' etc." This would require us to pronounce the name 'Ar'-karn-sar' (not dwelling on the *r*) or 'Ah'-kahn-sah.' Mr. Hill pleads for consistency in pronunciation (!): is *he* consistent? How can he be when he gives three distinct values for the *a*'s in 'Arkansas'? If the last *a* should be sounded as *aw* in 'law,' consistency would require us to say 'Aw'-kawn-saw.' The final '-saw' hardly represents the common pronunciation of early writers, as there was a great diversity. We find, 'Acan*sea*,' 'Acan*sias*,' 'Acanc*es*,' 'A Kanc*ea*,' 'A Kans*aes*,' 'A Kans*e*' (Marquette's 'A Kans*ea*,' Jefferys' 'A Kans*sis*'), etc. All of these will appear hereafter in 'Indian Synonymy,' when published by the Bureau of Ethnology.

Though not a New Englander, I propose to adhere to 'Ar-kan'-sas' when speaking the English name, and 'A'-kan-sa' when I use the Indian one, though I run the risk of being thought inconsistent.

As to 'Kansas,' how can Mr. Hill say that 'Kans*aw*' was the early Anglo-American pronunciation, when he gives Long's 'Konza' (i.e., 'Kon'-zay' or 'Con'-zay') as an approximation of the true pronunciation? 'Kan'-ze' (*n* a vanishing nasal, *a* as in 'father,' *e* as in 'they') is the name of the Kansa, Kansas, or Kaw tribe, as given to me by the Indians themselves. This agrees with what I have gained from cognate tribes, the Omahas, Ponkas, and Osages. The early French forms of the name are 'Canzé' (1722), 'Cansez' (1701?), 'Canses' (1702), 'Canzez' (1758), 'Canzas' (1774), 'Kancas' (1753), 'Kansé' (1722), 'Kanses' (1702). Early Anglo-American forms are 'Cansa' (1705), 'Kansæ' (1741), 'Kanzas' (1695), 'Kansez' (1761), 'Kanses' (Pike), and 'Kar'-sa' (LEWIS and CLARKE, *Discov.* 1806, p. 13).

The Quapaws or Kwapa say that they were originally part of the Kansas, and the former are the same as the Akansa. Query: was 'A Kansa' or 'A Kanze' ('A-Kan-sæ,' Coxe, 1741) derived from 'Kanze'?

There has been a tendency on the part of some Americans to change the Indian *a* as in 'father,' and *e* as in 'they,' to *aw* as in 'law.' Thus: 'U-ga'-Khpa' ('Oo-gŏkh'-pah') is now 'Qu*aw*-p*aw*,' or 'Quap*aw*;' 'Wa-zha-zhe' (War-zhar'-zhay'), or 'Osage,' is given as 'Was-ba-*shaw*;' 'Pan'-ka' ('Pahn'-kah'), as 'Pŭn-*caw*;' and 'U-ma'-ha' ('Oo-mah'-hah') as 'O-m*aw*-h*aw*.' So 'Arkans*aw*' and 'Kans*aw*.' I protest against such cacophonies, which are neither English nor Indian. When the regular Indian pronunciation of a word cannot be retained, let us use one that is euphonic English. J. OWEN DORSEY.

Bureau of Ethnology, Washington, D.C., Jan. 3.

Cheyenne.

MR. WILSON says (*Science*, Nov. 11, 1887, p. 239) that *Shah-ee-aie loo-hah*, said by the Dakotas to the first Cheyennes met by them, means 'you have painted yourselves red.' Its real meaning is, 'you have or possess (*loo-hah*) a Cheyenne (*Shah-ee-ay-lah*).' *Lu-ha* (*loo-hah*), 'you have' or 'possess,' is from *yu-ha* (*yoo-hah*), which cannot be used as an auxiliary in forming the perfect tense (for which there is no exact Dakota equivalent). 'You have painted yourselves red' must be expressed by *shah-nee'-ch'ee-yah'-pee*, in which *shah* is 'red;' *nee-ch'ee*, reflexive pronoun, second person; *yah*, causative; and *pee*, the plural ending. J. OWEN DORSEY.

Bureau of Ethnology, Washington, D.C., Jan. 3.

The Eskimo Ring-Finger.

WE found the habit of wearing finger-rings quite general among the Eskimo of Point Barrow during the two years we spent among them (1881–83). These rings are generally made of brass, rarely of silver, and it was quite natural to suppose that they learned the fashion from American whalemen. The ring, however, is always worn on the middle finger, and indeed received its name (*katûkqlerûñ*) from *katûkqlûñ* ('middle finger'), corresponding to the Greenlandic *kiterdlek* (literally 'the middle'). This circumstance was supposed to be merely accidental, especially as the word used in modern Greenlandic for ring does not indicate any particular finger, meaning simply 'the thing which belongs on a finger' (*agssangmio*).

The use of rings is not mentioned, as far as I can tell, by any writers who have described the Eskimo (though *agssangmio* occurs in Kleinschmidt's Dictionary), and every thing favored the belief that the fashion was merely local at Point Barrow and in Greenland (and possibly elsewhere), and had been learned after they had come in contact with civilized people.

'I was not a little surprised, therefore, when I had an opportunity of consulting the earliest Eskimo dictionary (that of Paul Egede, published in 1750), to find given as a derivative of the word *kiterdlek* (which, by the way, appears in the form *katertlek*, decidedly nearer the Point Barrow pronunciation), *katertleraut* ('a ring:' "annulus, quia Groenlandi annulum in *medio* digito gestare").

Whatever may be the fashion nowadays in Greenland, it is quite plain that in Egede's time the Greenlanders, like their more unsophisticated cousins at Point Barrow, not only wore the ring on the middle finger, but named it from that finger.

Moreover, the word for 'ring' in the Mackenzie River dialect (*kpitep-klopon*) indicates a similar fashion in that region. Such a coincidence in widely separated branches of the same race could hardly be the result of accident. Nor is it easy to see how any circumstances of environment could have affected such a trifling matter as which finger a ring should be worn on.

Evidently, therefore, before the Eskimo had separated into their present branches, they ornamented their hands with rings, which they wore on the middle finger, and not on what the white race have for ages considered as the ring-finger.

The question of the position of the ring-finger may appear, as I have called it, a trifling matter; but I think I have shown it to be a link in the chain of evidence connecting the different branches of the Eskimo race, and, as such, worthy of consideration.

JOHN MURDOCH.

Smithsonian Institution, Jan. 4.

Queries.

22. WASP-STINGS. — I have often, from childhood to the present time, heard the assertion that while one holds his breath it is impossible for him to be stung by a wasp. I have till recently always dismissed the assertion with the same smile that I have the statement that swallows hibernate in the mud, or that Friday is an unlucky day. My only reason now for asking place in the columns of *Science* for a question concerning it is the persistent assertion, made by a gentleman of the highest intelligence, whose opinions and judgment are of recognized value in scientific as well as other departments of thought, that the statement is true. Unfortunately, my own experiments have only been with wasps that were rendered somewhat torpid by cold weather, and count for nothing either way. I cannot learn that similar claims are made in regard to bees or hornets; nor can I learn, from those who make them in regard to wasps, whether it is claimed that the act of holding the breath renders one's skin impervious to the wasp's sting, or whether it in some way changes the nature of the virus or of the sensitiveness of the flesh to it. The assertion simply is, that any one may, while holding the breath, handle the liveliest and most able-bodied wasps with perfect safety, and also without after-pain or ill effect from any efforts of the wasp made while respiration was suspended. Can any readers of *Science* prove or disprove these assertions, and, in case they are sustained, give any theory whatever in explanation? C. H. AMES.

Boston, Dec. 28.

SCIENCE.—SUPPLEMENT.

FRIDAY, JANUARY 13, 1888.

ADDRESS OF MAJOR POWELL IN MEMORY OF PROFESSOR BAIRD.[1]

BAIRD was one of the learned men of the world, and, to a degree perhaps unexampled in history, he was the discoverer of the knowledge he possessed. He knew the birds of the air, from the ptarmigan that lives among everlasting snows, to the hummingbird that revels among the orchids of the tropics; he knew the beasts of the forests and the prairies, and the reptiles that crawl through desert sands or slimy marshes; he knew the fishes that scale mountain-torrents, that bask in quiet lakes, or that journey from zone to zone through the deep waters of the sea. In all this realm of nature he had a minute and comprehensive knowledge that no other man has ever acquired. What others have recorded in this field of research he knew, and to their discoveries he made a contribution of his own so bounteous, so stupendous, that he is recognized as the master of systematic zoölogists.

All of Baird's scientific work is an illustration of modern inductive or scientific reasoning. The inductions or general principles of modern science are reached by the accumulation of vast stores of facts. He knew how to accumulate facts; how to reject the trivial and select the significant. Modern science is almost buried under the *débris* of observation, the record of facts without meaning, — the sands of fact that are ground from the rock of truth by the attrition of mind; but Baird could walk over the sands and see the diamonds. Then he knew how to marshal significant facts into systems, and to weld them into fundamental principles. In all his works there can be discovered no taint of *a priori* reasoning or syllogistic logic; for in his mind there was no room for controversy, and disputation fled before the light of his genius. Formal logic, a disease of modern thought, — the contagion of Aristotlina, — never ravaged his brain. With healthful directness, he sought the truth guided by wise inference, and told the truth in its simplicity.

Baird was an organizer of the agencies of research. When a bold explorer essayed to penetrate the seas of ice by the path of peril and in quest of fame, he would ever so manage that a corps of quiet scholars should be attached to the expedition to study the climate of the Arctic zone, the geology of the Arctic rocks, the flora of the Arctic lands, or the fauna of the Arctic fields; and the best knowledge we have of the igloo-dwellers, the Eskimo whose home is on the ice of the north, has been brought to us by the quiet students he succeeded in attaching to Arctic exploring expeditions; and so the love of glory was made to serve the cause of truth.

When, in the interests of international commerce, expeditions were sent to explore and survey routes of travel and transportation across Central America from sea to sea, he managed to send with them corps of scientific men whose function it was to bring from the tropics all forms of its abundant life, vegetal and animal, and the relics of the arts of the people of Central America as they are exhibited in stone and clay and gold; and the National Museum has been enriched by the results of this labor, and the boundaries of human knowledge extended thereby; and so the greed of gain was made to serve the love of truth.

When our army was distributed on the frontiers of the land, he everywhere enlisted our scholarly officers into the service of science, and he transformed the military post into a station of research, an Indian campaign into a scientific expedition. Scott, Marcy, McClellan, Thomas, and many other of the great generals of America, were in their younger days students of natural history, and collectors for Baird. When our navy cruised around our shores, its officers were inspired with that love of nature which made every voyage of military duty a voyage of discovery in the realms of natural science; when they journeyed among the islands of the sea, they brought back stores of scientific materials; and when they sailed through the littoral waters of other continents, they made voyages of scientific investigation. Many of these earlier naturalists of the navy in subsequent times became commodores and admirals.

But time would fail me to tell of the exploring expeditions and the railroad surveys throughout America, and the travels throughout the world, which he utilized in the interest of science, or of which he was the immediate projector. Of the abundant material thus gathered from all parts of the world, some has gone to enrich American institutions of learning, and some has been gathered into the National Museum, — the result of Baird's organizing genius and a splendid monument to his memory.

The hills of the land stretch not so far as the billows of the sea; the heights of the mountains are not so great as the depths of the ocean; and so the world was unknown until this greater region was explored. The treasures of the land did not satisfy the desires of Baird, he must also have the treasures of the sea; and so he organized a fish commission, with its great laboratories and vessels of research.

> "What hidst thou in thy treasure-caves and cells,
> Thou hollow-sounding and mysterious main?
> Pale, glistening pearls and rainbow-colored shells,
> Bright things which gleam unreck'd of, and in vain.
> Keep, keep thy riches, melancholy sea!
> We ask not such from thee."

What the scholar asked of the sea was all its forms of life, its organisms minute and lowly, its crawling articulates, its pearl-housed mollusks, its fishes that swim in armies, and its leviathans that prowl among the waves, — the life of the reedy shore, the life of the ocean-current, and the life of the deep sea. So, with many ingenious appliances, he and his lieutenants sailed away to explore the ocean's mystery. So the Fish Commission was an agency of research; but it was more: he made it an agency by which science is applied to the relief of the wants of mankind; by which a cheap, nutritious, healthful, and luxurious food is to be given to the millions of men. He affirmed that for the production of food an acre of water was more than equal to ten acres of land, thus giving to the gloomy doctrine of Malthus its ultimate refutation, and clearing away the veil of despair from the horizon of the poor; for, when the sea shall serve man with all the food that can be gathered from its broad expanse, the land will not contain the millions whom it is thus possible to supply.

In the research thus organized the materials for the work of other scientific men were gathered. When a great genius reads to the world a chapter from the book of nature, the story is so beautiful that many are stimulated to search in the same field for new chapters of the same story. Thus it was that the publication of Baird's great works on natural history developed in America a great corps of naturalists, many of whom have become illustrious; and the stimulus of his work was felt throughout Europe. In the research which he organized the materials were furnished for this corps of naturalists; but his agency in the development of this body of workers was even more direct. He incited the men personally to undertake and continuously prosecute their investigations. He enlisted the men himself, he trained them himself, he himself furnished them with the materials and instruments of research, and, best of all, was their guide and great exemplar. Thus it was that the three institutions over which he presided, the Smithsonian Institution, the National Museum, and the Fish Commission, were woven into one great organization, — a university of instruction in the methods of scientific research, including in its scope the entire field of biology and anthropology. Such is Baird the investigator, Baird the organizer, and Baird the instructor, in the length, breadth, and thickness of his genius, the solidarity of a great man.

All that I have said is a part of the public record; it is found in

[1] Delivered Jan. 11, before a joint meeting of the Philosophical, Biological, and Anthropological Societies of Washington.

the great libraries of the world. But, however exalted the feeling of admiration we may entertain for Baird as an eminent scientific man, it is to his attributes as a man as disclosed in his personal relations with friends, associates, and contemporary men of affairs, that we most fondly turn, since it is in these relations that he most clearly exhibited those kindly and modest traits of character which made him so universally beloved.

As a man of affairs, Professor Baird exhibited great sagacity. His plans for the organization of scientific work were of great magnitude; and had they been presented to the administrative officers of the government or to legislative bodies with exaggeration, or even had they been presented with the glow of an enthusiastic missionary of science, they might well have encountered opposition. But Baird had a wonderful faculty of presenting his plans with extreme modesty, and with a degree of under-statement but suggestive explanation of possibilities which speedily caused him to whom the appeal was made himself to become an advocate of the professor's measure. He had traits of character in this respect which are hard to explain, and which seem at first to be contradictory. In the advocacy of measures his modesty amounted almost to timidity, and he avoided alike argumentation and notoriety, and he presented his measures with the directness of a child.

Notwithstanding all this, there was such a poise of faculties, such dignity of mien, that he impressed those with whom he came in contact as a venerable and wise patriarch. He seemed devoid of personal interest or feeling, and solicitous only for the welfare of those to whom he was in fact appealing, and he conveyed the impression that he was giving benignant advice. Thus the shrinking, sensitive man, who could not even stand before a public body, such as a committee of Congress or a scientific society, and advocate a cause, could, from his seat by the fireside or at the desk, so illumine the subject with which he had to deal that men stood round him to gather his words, that nothing should be lost; for in the exposition of his subject he illumined ever thing with clear statement, arising from an exhaustive knowledge and full understanding of results.

As the director of the work of research in which other men were engaged, Professor Baird had marvellous insight and skill. The appliances of modern research, alike in the inorganic world and in biology, have come to be multifarious and diverse; and there is this peculiarity about their use: that once used, so that the secret of nature which they were planned to unlock has been revealed, they speedily become obsolete, and immediately new keys, new apparatus, new devices, are necessary. Thus to a very large extent skill in research is absorbed in the skill necessary for the development of the agencies of research. A continuous line of research, prosecuted by a corps of men so that the boundaries of knowledge are carried far forward, can result only from a continuous line of inventions in the apparatus of research; and it was here that Baird exhibited his skill. His own devices were many and constant, and even he was fertile in suggestions to his assistants. No wonder, then, that so many of the secrets of nature were unlocked through his agency. It was in the direction of this work of research that the man Baird stood forth as a giant; it was where his vast knowledge of details was most apparent; it was where his marvellous skill was most shown; it was where his insight into human character was most exhibited. With clearness he formulated his interrogatories; with aptness he selected his course of procedure; with judgment he sought the aid of others, and with suggestiveness directed their work. And, lo! his questions were speedily answered. It was in this manner that his own good hands were supplemented by the hands of many, that his own great mind was re-enforced by the best mental activity of many assistants; and thus the whole body of men under his control worked together as one organic integer for the increase and diffusion of knowledge among men.

In his work with his assistants he scrupulously provided that every one should receive the meed of honor due for successful research, and treated all with generosity. Many an investigation begun by himself was turned over to assistants when he found that valuable conclusions could be reached; and these assistants, who were his warm friends, his younger brothers, reaped the reward; and he had more joy over every young man's success than over the triumphs and honors heaped upon himself from every quarter of the globe. He was the sympathetic counsellor of many men; into his ears were poured the sorrows and joys of others, and he mourned with the mourning and rejoiced with the rejoicing. To those in need his hand was ready and his purse was open, and many and many were the poor who called him 'blessed.' Though a man of great force of character, a man of great learning, a man upon whom had been showered the honors of the scientific world, in character he was as simple as a child. He had a fund of 'folk-lore,' and loved the books and papers written for children. In his later years, weakened with disease and burdened with many labors, he still read *St. Nicholas* from month to month, and kept the run of every little story, and was glad to be 'a child again.' His life at home was pure and sweet, and full of joys, for he gave and received love and trust and tender care. But the history of his home life is sacred. Its words and acts abide in the hearts of the husband, the wife, and the daughter.

For many long months he contemplated the day of parting. Labor that knew no rest, responsibility that was never lifted from his shoulders, too soon brought his life to an end. In the summer of the past year he returned to his work by the seaside, that he might die in its midst. There at Wood's Holl he had created the greatest biologic laboratory of the world; and in that laboratory, with the best results of his life-work all about him, he calmly and philosophically waited for the time of times. Three days before he died he asked to be placed in a chair provided with wheels. On this he was moved around the pier, past the vessels which he had built for research, and through the laboratory, where many men were at work at their biologic investigations. For every one he had a word of good cheer, though he knew it was the last. At the same time, along the pier and through the laboratory, an invalid child was wheeled. "We are rivals," he said, "but I think that I am the biggest baby." Then he was carried to his chamber, where he soon became insensible, and remained so until he was no more.

"Blessed are the pure in heart, for they shall see God."